

EDWAA IMPACT EVALUATION DESIGN SUMMARY

The net impact evaluation of the Economic Dislocation and Worker Adjustment Assistance (EDWAA) program is intended to provide policy-relevant information about the efficacy of the EDWAA program in improving the reemployment prospects and earnings of dislocated workers--that is, workers permanently separated from their prior employers. It will provide information about whether the adjustment services and retraining provided through EDWAA are effective in improving labor market outcomes, as well as about which groups of dislocated workers benefit most from these services and retraining. This information will be provided through a rigorous random-assignment evaluation.

This document describes major elements of the design for the evaluation. It begins with a description of the objectives of the evaluation. It then describes the sample design--the sample frame, the treatment and control groups, random assignment, sample sizes, and the selection of sites. It concludes with a discussion of the implications of this design for what will be learned.

OBJECTIVES OF THE EVALUATION

The main objectives of the evaluation are to estimate the net impact of EDWAA services on the post-program employment and earnings of dislocated workers and to estimate the separate impacts of the two major EDWAA services--basic readjustment services (BRS) and retraining. Secondary objectives are to examine impacts on the characteristics of post-program jobs including wage rates and fringe benefits, to describe the reemployment services and retraining received through EDWAA and to compare these services to those received by control group members, to measure client

satisfaction with the services, and to examine impacts on important subgroups. Important subgroups include individuals identified through rapid response activities, individuals whose layoff was part of a mass layoff, older individuals, individuals with low education levels, and individuals with income support (for example, from unemployment compensation or their spouse's earnings).

SAMPLE DESIGN

Sample Frame: All EDWAA applicants who are determined to be eligible for EDWAA and who apply for EDWAA services through substate area (SSA) program operators will be included in the sample frame if they receive services funded through substate grants and state reserves. Ideally we would also like to include individuals who receive services through national reserve account grants since this source of funding represents a significant fraction of EDWAA spending nationwide. However, this may not be possible since these grants generally provide services to workers laid-off from specific plants and funds can be spent over a three year period. These circumstances make it difficult to implement a design which offers groups of workers different levels of services and which enrolls a sample in a fixed time period (of approximately a year).

Treatment/Control Group Design: Because we are interested in estimating the impacts of both the overall EDWAA program and its two major components--basic readjustment services (BRS) and retraining, the design will include two treatment groups--(1) the full array of EDWAA services, (2) BRS only--and a control group (no EDWAA services). Random assignment to both treatment groups and the control group will occur at the same point in the application process. Individuals assigned to the "EDWAA treatment" will be eligible for the full array of EDWAA services including basic readjustment services and retraining while the individuals offered BRS only will be eligible for all services except for retraining.

Under this design an estimate of the overall impact of EDWAA can be obtained by comparing mean outcomes of the control group with mean outcomes of the EDWAA (basic readjustment services and retraining) treatment. Estimates of the effects of the two main EDWAA services (basic readjustment services and retraining) can be obtained directly through comparisons among treatment and control groups. The difference in outcomes for the two treatment groups divided by the retraining participation rate will provide an estimate of the impact of retraining relative to providing only basic readjustment services. The difference in outcomes for the basic readjustment services only treatment group and the control group will provide an estimate of the impact of providing basic readjustment services.

In developing this design we considered two alternative designs. The first alternative was to have a single treatment group that was offered all EDWAA services and a control group that was not offered EDWAA services. This design would provide an estimate of the overall impact of EDWAA. Estimates of the separate impacts of the major EDWAA services (BRS and retraining) could also be obtained, but they would have to rely on our ability to model participation in these services. It was decided that the two treatment group option was superior since estimates of the separate impacts of services could be obtained directly from treatment-control comparisons and since the loss of precision resulting from dividing the treatment group into two separate groups was modest.

The second alternative was to have a single treatment group offered all EDWAA services and a control group that was offered EDWAA basic readjustment services. This design would focus the evaluation on providing an estimate of the impact of EDWAA retraining rather than an estimate of overall program impacts. While it was recognized that this design only provided a partial examination of EDWAA impacts, it was proposed because many dislocated workers are likely to receive adjustment services from non-EDWAA sources, making it difficult potentially to measure the effects of EDWAA basic readjustment services relative to those of other adjustment services. However, it was decided that EDWAA funded

readjustment services are sufficiently more intense than the services typically provided through other sources and that EDWAA basic readjustment services are likely to have a measurable impact on employment and earnings even though control group members receive some adjustment services. Hence we decided that it was not necessary to narrow the design to focus on examining the impact of retraining only and that we could measure the impact of the entire program.

Random Assignment: Random assignment will take place when individuals apply for and are determined to be eligible for EDWAA services.¹ Some services funded by EDWAA through rapid response, profiling, or one-stop career centers are likely to occur prior to random assignment.

We selected the application and eligibility determination point for random assignment because it is the earliest feasible point to conduct random assignment. Selecting this point for random assignment minimizes the likelihood that control group members will receive EDWAA funded adjustment services although, as noted above, some services that do not require an application for EDWAA may be received prior to this point.

Some EDWAA applicants will find jobs before they actually receive substantial EDWAA services. Given this, using an early point for random assignment as opposed to a later point, say after enrollment and assessment, is likely to reduce observed treatment-control differences in outcomes because many treatment group members would not receive services. This may jeopardize our ability to detect treatment-control differences for a given sample size. One approach to minimizing this problem is to conduct random assignment at two points. At the application point some individuals would be assigned to a control group that is not offered EDWAA services and at a later point--after enrollment and assessment or after a service plan is developed--some individuals would be permitted to receive retraining while the remaining

¹Random assignment procedures will be adapted to each site's application process.

individuals would not be permitted to receive retraining. This approach would enhance our ability to measure the impacts of retraining by maximizing the retraining participation rate in the sample. On the other hand, this approach would complicate the random assignment process by requiring two rounds of random assignment. It would also potentially place site staff in the difficult position of denying retraining to individuals whose assessment/service plan recommended retraining. For these reasons we rejected a two step random assignment process.

Sample Size/Number of SSAs: We expect to select a sample of approximately 10,500 EDWAA eligibles for the evaluation. We plan to select these individuals from 30 substate grantees (SSAs) throughout an approximately one year intake period. We will randomly assign one-third of them to each of the two treatment groups and we will assign the remaining third to the control group. We plan to collect baseline information on all of these individuals (through the EDWAA application and a supplementary baseline form) and administrative records data (SPIR, wage records, and possibly UI and ES data). We also plan to conduct follow-up interviews with a subsample of about 4,300 sample members.

These sample sizes are chosen to allow us to detect cost effective impacts of EDWAA services. Specifically, we estimate that an impact of approximately \$300 on quarterly earnings would be cost effective. We also estimate that, for estimates made with wage records data, the minimum detectable difference in quarterly earnings between each treatment group and the control group is \$289 for estimates that can be generalized to the entire EDWAA program (externally valid estimates). The minimum detectable difference for estimates that can be generalized only to the sites in the demonstration (internally valid estimates) is \$195. Although minimum detectable differences for estimates made with the follow-up survey data will be larger because of the smaller sample size, the minimum detectable difference (\$263) for internally valid estimates still meets our \$300 precision standard. While the minimum detectable difference (\$337) for externally valid estimates is larger than this precision standard, it is still relatively close to the

standard. Moreover the minimum detectable difference for the estimate of the overall impact of the EDWAA program can be lowered to \$322 by combining the two treatment groups.²

These precision estimates and our conclusions about the sample design are based on several assumptions and decisions that are worth reviewing. First, we established a precision standard (that we wanted to be able to detect a \$300 impact on quarterly earnings) to guide our decisions about the sample size required for the evaluation. This precision standard was based on assumptions about the cost of services and the period over which program impacts would persist that were based on existing data and past experience. If true costs exceed our estimates and program impacts are of shorter duration, our precision standard will be more than adequate because we would need to observe impacts that are larger than our precision standard to conclude that program benefits exceed costs. However, if program costs are lower than we assumed or impacts continue for a longer period, our precision standard will be too high and hence our sample size will be too small.

Second, we have proposed a two-stage process for selecting a nationally representative evaluation sample in which we randomly select 30 SSAs in the first stage and randomly select individuals in the second stage. A one stage design in which individuals are selected directly would yield more precise estimates for a given sample size, but there is no nationwide list of eligible applicants that could be used to select such a sample. For this reason we have proposed selecting SSAs first and then selecting individuals. However, by clustering the sample in a small number of SSAs we must account for the variance in impacts among SSAs when examining the statistical power of alternative designs. We have done this in the calculations and concluded, based on our assumption about cross-site variance, that we should select 30 sites for the evaluation.

²This estimate accounts for the fact that the sample created by combining the two treatment groups overrepresents individuals who receive basic readjustment services as opposed to retraining.

Third, we have proposed a design in which we select SSAs with probability proportional to size and in which we then assign all eligible applicants to the evaluation treatment and control groups over a one-year in-take period. This design yields unequal sample sizes per SSA which is a less efficient design than a design that has equal sample sizes per site. However, because there is wide variation in the size of individual SSAs we believe this design makes sense. In PY94 the average SSA reported 186 terminees, but one third of all terminees came from the 411 SSAs with less than 180 terminees, one third came from the 137 SSAs with 180 to 415 terminees, and one-third came from the 50 SSAs with over 415 terminees. If we drew equal size samples from each SSA, we would be restricting the overall sample size to the number we could obtain in the smaller SSAs. Thus we have decided to select all individuals from each SSA in the evaluation and to obtain records data on all these individuals, since the cost of obtaining administrative records is driven primarily by the number of sites not the number of individuals. However, we plan to allocate the followup interview subsample equally by site to the extent feasible. Finally the calculations assume that we will obtain an average of 100 treatment group members and 50 control group members from the smallest sized group of SSAs, 200 treatment group members and 100 controls from the middle sized group of SSAs, and 400 treatment group members and 200 controls from the largest sized group of SSAs. Based on the numbers for PY94 these assumptions are likely to be conservative and the final sample size will be larger if we are successful in recruiting 30 sites.

Finally, we have assumed in our calculations that we will assign one-third of the sample to each of the two treatment groups and one-third to the control group. This is the most efficient design for comparing each treatment group with the control group and for comparing the two treatment groups. An alternative would be to limit further the number of controls to say one-quarter of the overall sample. We have examined the implications of this alternative for the precision of the estimates under the assumption that we still obtain 100 treatment group members from the smallest sites, 200 from the middle sized sites, and 400 from the

largest sites, but that we reduce the number of controls to make the total control sample one-quarter of the total sample.³ Under this assumption the overall sample size would be reduced to 9,320. The minimum detectable treatment control differences would be \$304 and \$218 for the externally and internally valid estimates of quarterly earnings impact estimates. These differences also satisfy our precision standard.

Site Selection: SSAs will be selected randomly with probability of selection proportional to size. We also plan to stratify sites geographically and by size. We expect that geographic stratification will insure that we have adequate representation of different approaches to service delivery and different populations of dislocated workers.

IMPLICATIONS OF THE DESIGN

The proposed design will provide nationally representative estimates of the impacts of EDWAA and its two main components--BRS and retraining. However, because of the complex nature of the EDWAA program the proposed evaluation will not be able to measure the impacts of all EDWAA funded activities nor will it be able to measure the impacts of services funded through non-EDWAA funding mechanisms.

Rapid Response: The design will allow us to measure impacts on individuals who are identified through the rapid response mechanism and who are subsequently offered BRS or retraining. This analysis will answer the question as to whether we have larger impacts for individuals identified early in their layoff spell, which is a major purpose of rapid response, but it will not allow us to assess the full impacts of rapid response. For example, individuals may come to an initial rapid response session and receive information on the local labor market and this information may help them find a job. We will not measure the impact

³Specifically we assumed that we would obtain, respectively, 33, 66, and 133 controls from the three size groupings of sites.

of this up-front service since such individuals will not be part of the sample because they never apply for EDWAA.

Profiling: In many states individuals are referred to EDWAA by Worker Profiling and Reemployment Services (WPRS). We may not be able to measure EDWAA's impact on these individuals because some of the individuals receiving WPRS services will not stay in contact with EDWAA long enough to be randomly assigned and therefore cannot be included in the study. DOL is, however, funding an evaluation of WPRS which will provide impact estimates for these services.

One-stop career centers: In some locations EDWAA funds are used to support one-stop career centers. As with the funds used for profiling we will not be able to measure the impact of these funds because many individuals receiving services from these centers will not stay in contact with EDWAA long enough to be randomly assigned. However, we will be measuring the impact of basic readjustment services when delivered explicitly by EDWAA. To the extent that the services delivered by one-stop career centers are similar to basic readjustment services, we will have a measure of impacts for this type of service.

Trade Adjustment Assistance (TAA): Some sample members of both the treatment and control groups will be eligible for and receive TAA funded services. However, it is very unlikely that we will be able to identify the separate effect of TAA services since the number of EDWAA applicants receiving services from TAA will be quite small and since we are not able to use random assignment for an entitlement program like TAA.